On these bases the following table is calculated :—

Excess of the mean stature of the pedigree breed in each successive generation, above that of the rest of the population, when both parents occupy the undermentioned grades in their own generation of the pedigree-breed.

Generation.	99°	95°	90°	80°	70°
	inches.	inches.	inches.	inches.	inches.
1	3·9	2·8	2·2	1·4	0·9
2	6·1	4·3	3·4	2·2	1·4
3	7·5	5·3	4·1	2·7	1·7
4	8·4	6·0	4·7	3·1	1·9
5	9·1	6·4	5·0	3·3	2·1
&c.	&c.	&c.	&c.	&c.	&c.
Limiting values ...	10·4	7·3	5·7	3·8	2·3

The importance to the breeder, of using highly selected parents, is *measured* by these tables, and shown to be very great. Thus one generation of the 99° selection is seen to be more effective than two generations of the 90° selection, and to have about equal effects with those of an 80° selection carried on to perpetuity. Two generations of the 99° selection are more effective than four of the 95°, and than a perpetuity of the 90°.

It must be borne in mind, that there is no stability in a breed improved under the supposed conditions ; but that, as soon as selection ceases it will regress to the level of the rest of the population through stages in which the deviation at starting, sinks successively to $w, w^2 \ldots w^n$ of its value. It may, however, happen that a stable form will arise during the process of high breeding, that shall afford a secondary focus of regression, and become the dominant one, if the ancestral qualities that interfere with it be eliminated by sustained isolation and selection. Then a new variety would, as I conceive, arise ; but into this disputable topic there is no need to enter now.

We can thus understand the facility with which races of butterflies acquire mimetic forms, the severity of selection in their case being very great, while one of their generations occupies only a year.

Francis Galton.

The Effect of Röntgen Rays on Liquid and Solid Insulators.

Owing to my absence from Cambridge in the Easter vacation, I have not until to day seen the paper by Lord Kelvin, Dr. Beattie and Dr. M. Smolan (Nature, March 25), on the influence of Röntgen rays on electric conduction through air, paraffin, and glass, in which the authors state that they cannot detect any influence of Röntgen radiation on conduction through solids. I think that the difference between this result and the one obtained by Mr. McClelland and myself arises from the temporary character of the effect of the radiation on solids. The increase in the conductivity of solids is only appreciable for a short time after the application of the electric force (see Nature, July 30, 1896, p. 306) ; under long-continued electromotive forces the conductivity seems unaffected by the rays. The effect might perhaps be more accurately described as an increase in the electric absorption, rather than as an increase in conductivity. I have been for the past few months engaged in experiments on the effect of the rays on solids and liquids, particularly liquids ; and, though the experiments have been much interrupted by the pressure of other work, I hope soon to have them ready for publication. There is one experiment, however, which may be of interest. Of all the liquids tried, that sold as vaseline oil has proved the best insulator ; in its pure state it is very transparent to Röntgen rays, so to increase the absorption of these rays I stained the oil with iodine, when it became very opaque to them. The oil does not insulate so well after staining as it did before, but the effect of a slight amount of conductivity is not of importance when the following method is used. Three electrodes, A, B, C, are placed in a leaden vessel filled with the oil. B, which is between A and C, is connected to one pair of quadrants of an electrometer, A and C to the terminals of a battery of 1000 small storage cells. If there is any leakage the potential of B will, in general, not remain zero after the battery is put on, but it will do so if an earth connection is made at the proper place in the battery. The base of the vessel below B C was cut out, and an aluminium vessel inserted, so that the liquid between B and C could be exposed to the Röntgen rays. A balance was obtained with the rays off ; when the rays were turned on, the potential of B no longer remained zero, but changed in the way it would if the conductivity between B and C had increased. This effect was small but well marked, and seemed to last however long the electromotive force was kept on.

J. J. Thomson.

Cavendish Laboratory, April 24.

The Theory of Dissociation into Ions.

Mr. Spencer Pickering has, in your number for January 7, brought forward certain difficulties which he says the advocates of the dissociation hypothesis have persistently ignored. I have been waiting in the hope that some one who supports the gaseous theory of solution as well as the theory of electrolytic dissociation would answer his letter. As no one has done so, I venture once more to trespass on your space.

First let me say that the experiment described by Mr. Pickering, in which water or propyl alcohol exudes through the walls of a semi-permeable vessel containing a mixture of these liquids, according as propyl alcohol or water is placed without, appears to me, as it does to him, to be very strong evidence that it is complex molecules of solution to which the walls are impervious. The experiment is one which certainly needs explanation at the hands of those who uphold the gaseous impact theory of osmotic pressure.

As I have already said, the idea that electrolytic conductivity depends on dissociation of the ions from each other, does *not* involve, as is so often assumed to be the case, the gaseous view of solution. The evidence for such dissociation appears to me to be exceedingly strong, as I will explain very briefly below, so that some explanation of the second experiment described by Mr. Pickering is necessary.

The experiment is this : The freezing point of a large quantity of acetic acid, to which is added a mixture of sulphuric acid and water in the proportions represented by $100H_2O + H_2SO_4$, shows that considerably less than 100 molecules have been dissolved. This result indicates that chemical union has occurred. Mr. Pickering says that, on the dissociation theory, the freezing point should be lowered by an amount corresponding to something between 101 and 103 molecules.

In such a case, however, we have conditions very different from those which hold when sulphuric acid is dissolved in water. In fact the liquid is in reality a mixed solution of water and sulphuric acid in acetic acid, or possibly, as Mr. Pickering suggests, of the hydrate of sulphuric acid in acetic acid. It does not at all follow that because sulphuric acid is dissociated in water, it is, therefore, dissociated in other solvents ; in fact, the freezing points of its solutions in acetic acid show that, on the contrary, aggregation has occurred. We should, therefore, expect that dissolving sulphuric acid in acetic acid would have little or no effect on the conductivity ; and this is also indicated by the low specific inductive capacity of acetic acid, which implies a low ionising power. There is no reason to suppose that the presence of a small quantity of water would modify the properties of the solvent enough to cause any appreciable change in the conditions.

But, even if these considerations were insufficient to explain the facts, the dissociation theory would not be discredited. As I pointed out in your issues of October 15 and December 17, 1896, dissociation of the ions from *each other* does not forbid the assumption that the ions are linked with one or more solvent molecules. Such a combination would explain Mr. Pickering's observation.

Mr. Pickering says that the dissociation theory depends solely on the numerical agreement obtained when properties of solutions are interpreted by its means. Although these numerical relations may have suggested the theory, they by no means furnish the only basis for it to rest upon. Other facts, to my mind, give much more conclusive evidence in its favour. As Mr. Pickering has challenged the supporters of the theory to explain his experiment, I may be allowed to ask the opponents of the theory to explain the following phenomena in any other way than by a dissociation of the ions from each other :—

(1) The velocity with which an ion travels through a dilute solution under an electric force is independent of the nature of the other ion present.

(2) The conductivity of a dilute solution is proportional to its concentration. The alternative to the idea of dissociation is to

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imagine that the ions work their way through the solution by a continual series of interchanges between the parts of two solute molecules when in collision. The frequency of collision, and therefore the ionic velocity, would then vary as the square of the concentration, so that the conductivity would depend on the cube of the concentration.

(3) The potential difference at the contact of two solutions of different concentrations has the value calculated on the assumption that the ions migrate independently of each other, so that the faster-travelling ion enters the neighbouring solution first, and gives it a charge which continually increases till the electrostatic forces prevent further separation.

It is such phenomena as these, and not the numerical relations between conductivity and osmotic pressure effects, which seem to me to offer the most convincing evidence in favour of the dissociation theory. W. C. Dampier Whetham.

Trinity College, Cambridge, April 24.

Mosquito-Bites.

An acquired immunity from the bites of mosquitoes and "domestic pests" is not uncommon in British India, and I have rejoiced in it myself, but should not trespass on you for space for details. I can give them to any of your correspondents who may care to ask me.

There is lying before me a queer old case of mosquito-bite reported by a good witness, Pedro Teixeira, who sailed from Malacca to Mexico in 1600 A.D., and crossed the latter from Acapulco to San Juan de Uluâ, on his way to Spain. Of this journey he says: "Almost all along this road is a plague of mosquitoes, so terrible and grievous that no defence avails against them, *and they stung my best slave to death.*"

102 Cheyne Walk, Chelsea, April 9. W. F. Sinclair.

THE NATURAL HISTORY OF WORMS.[1]

THIS is the third volume that has been issued of "The Cambridge Natural History." The previous volumes are vol. iii., Molluscs and Brachiopods (reviewed in Nature, lii. p. 149), and vol. v., Peripatus, Myriapods, and Insects (reviewed in Nature, liii. p. 322). In the multitude of the divisions in the animal kingdom with which it deals, the present volume differs considerably from its predecessors. It is true that one may even nowadays find most, if not all, of the many forms of life here described included in one heterogeneous section entitled Vermes; but the editors of the present book fully recognise the great distinction that exists between such forms as the Platyhelminthes or flatworms, the Oligochæta or earth-worms, the Rotifera, and the Polyzoa, and they have very wisely distributed the various sections to authorities whom every one will recognise as among the most competent to deal with their respective subjects. Indeed it may be questioned whether the separation of the subjects has not been carried a little bit too far. It is true that pages ix. to xii. contain what purports to be the scheme of classification adopted in the volume, but this is little more than a table of contents, in which no attempt is made to show the relationships of the orders or families mentioned; and, apart from this, the only bond of union between the various sections appears to be the quotation, very happily adapted from André de Chénier, "Nous allons faire des vers ensemble." We should much like to have seen some attempt on the parts of the editors to present their readers, all of whom are not supposed to be familiar with the newest ideas of zoology, with a short introduction showing how and why it is that this "old group of Vermes" has gradually been dismembered, so that now we find not only such forms as the Platyhelminthes and the Polyzoa claiming to rank as independent phyla of the animal kingdom, but we also find, in the scheme of classification at all events, the genus *Phoronis*, the few forms composing the Dicyemidæ and Orthonectida, and the thirty or so genera of leeches placed on the same high level. When we are told, as Mr. Gamble tells us, that the Turbellaria "occupy the lowest position in the whole group of worms," that they "are most closely allied to that great extinct group from which they, the Nemertinea, Rotifera, and even the Annelids, offer increasingly convincing evidence of having been derived," then we ask, What are we to

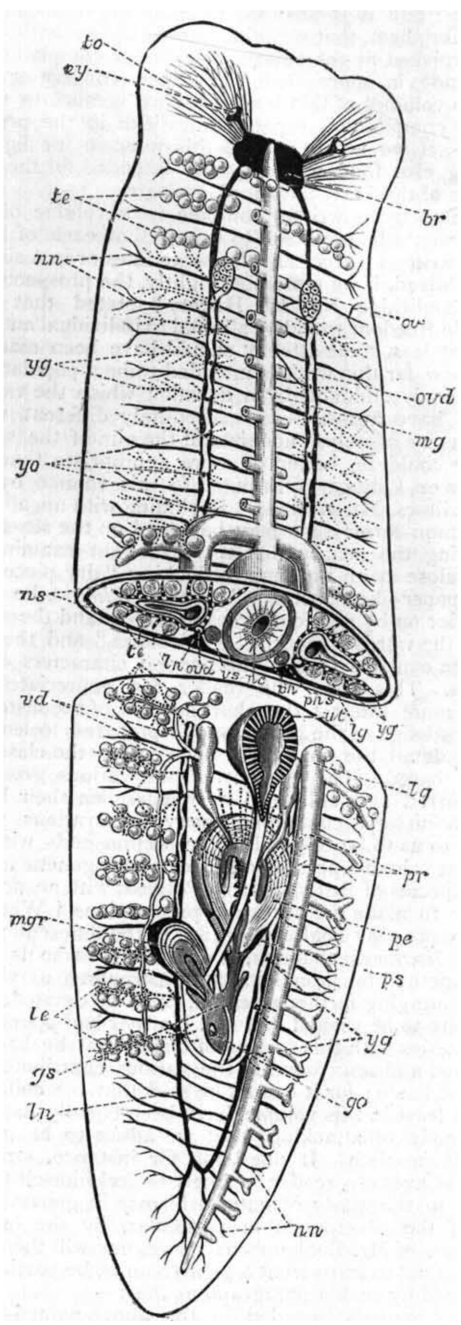

Fig. 1.—Diagrammatic view of the structure of *Planaria lactea*, a Turbellarian. × 7. The body has been cut across and a portion removed.

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[1] "The Cambridge Natural History." Vol. II.: Flatworms and Mesozoa, by F. W. Gamble; Nemertines, by Miss L. Sheldon; Thread-worms and Sagitta, by A. E. Shipley; Rotifers, by Marcus Hartog; Polychaet Worms, by W. Blaxland Benham; Earthworms and Leeches, by F. E. Beddard; Gephyrea and Phoronis, by A. E. Shipley; Polyzoa, by S. F. Harmer. The whole edited by S. F. Harmer and A. E. Shipley. 8vo. Pp xii. + 560; with numerous illustrations in the text. (London: Macmillan and Co., Ltd., 1896.)

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